passed the wider and higher would grow the tunnel in which you were groping your way. The walls of the tunnel would grow thicker at every step, and their thickness and stoutness would tell you that you were already in an artery, but the inside would be delightfully smooth. As you went on you would keep passing the openings into similar tunnels, but the further you went on the fewer they would be. Sometimes the tunnels into which these openings led would be smaller, sometimes bigger, sometimes of the same size as the one in which you were. Sometimes one would be so much bigger that it would seem absurd to say that it opened into your tunnel. On the contrary, it would appear to you that you were passing out of a narrow side passage into a great wide thoroughfare. I dare say you would notice that every time one passage opened into another the way suddenly grew wider, and then kept about the same size until it joined the next. Travelling onwards in this way you would, after a while, find yourself in a great wide tunnel, so big that you, poor little corpuscle, would seem quite lost in it. Had you anyone to ask, they would tell you that it was the main artery of the arm. Toiling onward through this, and passing a few, but, for the most part, large openings, you would suddenly tumble into a space so vast that at first you would hardly be able to realise that it was the tunnel of an artery like those in which you had been journeying. This you would learn to be the *aorta*, the great artery of all; and a little further on you would be in the heart."

In conclusion, we are sure that there is no book which could be more profitably placed in the hands of the youth of both sexes, as a means of intellectual training and general culture, than this small work of Dr. Foster's. It possesses the advantage of combining precise reasoning with information on a subject which is all-important in every-day life; a subject which, if more universally understood, would lead to the adoption, by all, of means for the healthy maintenance of life which are now as systematically ignored as they are misunderstood. The reader is referred to Prof. Huxley's "Elementary Physiology" for the discussion of many subjects which the space allowed and the age of the pupils make it necessary to omit in the work before us.

OUR BOOK SHELF

Exposition Géométrique des propriétés générales des Courbes. Par Charles Ruchonnet (de Lausanne). Troisième édition, augmentée et en partie refondue. (Paris, 1874.)

Eléments de Calcul approximatif. Par Charles Ruchonnet. Seconde édition augmentée. (Paris, 1874.)

WE have read these works with interest and somewhat of surprise: with interest because the subjects are fairly interesting and are treated in the well-marked style which distinguishes the writings of French mathematicians; with somewhat of surprise that the subjects treated at such length should have met with such a large circle of readers as is indicated by the number of editions that have been called for. The first work on our list establishes many general properties of curves by means of first principles and by the use of infinitesimals. This mode of treatment, so far as we know, is confined in our own text-books to a chapter or two in Dr. Salmon's works, and it would be hard to find more than he has given in any other work. The author himself states that this elementary knowledge will carry the student through the book with the sole exception that a more extended acquaintance with mathematics is required for an article devoted to the finding the distance between a curve and its osculating sphere in the neighbourhood of the point of contact. The author, too, claims the major part of the demonstrations as his own, though in some cases he has generalised results previously given, and in some cases has established known properties in a novel way.

The work is divided into two parts; the first treating of the tangency, curvature, and osculating circle of plane curves: the second part treats of the analogous properties for non-plane curves, and deals also with the polar surface, the osculating sphere, ruled surfaces, developables, and the osculating helix. There are five pages of plates containing eighty clearly drawn figures.

The "Calcul approximatif" is concerned with numbers only. M. Ruchonnet considers that he has improved upon the processes given by previous writers as regards their generality and precision as well as the facility with which they are effected. There are six articles and two notes. In the preliminary observations, the writer's aim is concisely stated to be the turning of an expression composed of incommensurable numbers (incommensurables avec l'unité) into a decimal to any given degree of exactness. He here treats of *absolute* and *relative* error, and then proceeds to summation. In the third article, in applying his methods to multiplication and involution, he sketches out the contracted process of multiplication employed by Oughtred; then follow contracted division (reference made to Serret's "Arithmétique"), evolution, and functions of a single variable. Amongst the important additions in this edition, is a complete solution of the problem "Combien de chiffres exacts faut-il calculer d'un nombre pour pouvoir en extraire la racine $m^{ième}$ avec n chiffres exacts?"

Many illustrative selections might be made, but as these would not be of general interest, we content ourselves with recommending those who take an interest in either of the subjects discussed by M. Ruchonnet to taste and judge for themselves.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Flight of Birds

IN NATURE, vol. x. p. 147, I observe a letter signed "J Guthrie," and dated from the Cape, on the subject of the Flight of Birds, and particularly on the "hovering" of birds. It appears that one of your correspondents had referred to my chapter on this subject in the "Reign of Law" as giving a satisfactory explanation of this phenomenon. Mr. Guthrie thinks, on the contrary, that what I have there said "requires no refutation;" which is not wonderful considering the entire misconception which he evinces of the explanation I have given. He quotes me as affirming that "by a proper arrangement of its wings and tail and the position of its body, a bird can, *without muscular exertion*, remain suspended in a horizontal air-current, provided the latter be of sufficient velocity." If I had said this I should have talked nonsense. But I have not said it, as your readers may see by referring to the page (170, first edition) to which Mr. Guthrie himself refers. What I have said is, that under certain conditions of strength of air-current a kestrel can maintain the hovering position "with no *visible* muscular motion whatever." Mr. Guthrie omits the word "visible," and probably has no idea of its force and meaning in the sentence referred to. The maintenance of the wings and tail in the proper position, and of the body at the proper angle, does in itself, of course, involve continuous and difficult muscular action, although it is not visible, just as a rope-dancer standing still in some tiptoe attitude may require immense muscular effort although no motion be visible, and although the whole aim, object, and effect of that exertion be to produce stillness, and not motion.

So far is it from being true that I have represented hovering as an accomplishment of wingmanship which requires little exertion, that I have asserted with emphasis the exactly opposite doctrine—that it is a specially difficult operation, requiring very often great exertion, and always requiring special muscular effort.

It is evident, however, that Mr. Guthrie is still ignorant of the facts which have to be explained. In the passage which he misquotes I am not stating any theory; I am stating a fact which I have seen over and over again. It is a fact beyond all question that a kestrel can maintain itself hovering in a strong horizontal air-current, with no other muscular exertion than that which is required to keep its wings and body at th right angle. I have seen it done a hundred times in level countries, when by no possibility could any upward deflection of the wind have arisen from the configuration of the ground.

One of the first and most fundamental facts to be admitted and accounted for in the flight of birds is, that perfectly horizontal air-currents have a powerfully sustaining effect upon vane surfaces, which are presented to them as birds' wings are presented. "Hovering" and "soaring" are only to be explained when this fact is seen and admitted. ARGYLL

Inverary, Argyllshire, July 30

Exhibition of Specimens and Apparatus at British Association Meetings

I AM anxious to draw the attention of the readers of NATURE to the arrangements to be made this year at the British Association meeting (for the first time) for the reception of specimens and apparatus illustrating papers or short communications made to the sections. The provision of a room for this purpose—a kind of temporary museum—has during the last four years been recommended by the committees of Sections C and D, several times, and this year the experiment is to be made. Those who have promoted this plan are naturally anxious that it should be a success. I would therefore appeal to the secretaries of the various sections to assist in initiating this new feature of the meeting, by endeavouring, as far as possible, to secure from the authors of papers objects which illustrate their communications; such objects to be deposited during the week of meeting in the room provided by the Council. This room will be open to inspection under the same regulations as the sectional meeting rooms, and the objects deposited will be carefully ticketed and arranged, and, where necessary, placed under glass cases.

From Section A we may expect physical and astronomical apparatus and models; from B, new chemical products and specimens of apparatus illustrating new processes; from C, geological specimens of rarity or new to science; from D, zoological and botanical specimens, anatomical preparations, for the exhibition of which microscopes will be provided, and also ethnological specimens; from E, maps and geographical models; from F and G, models or machinery not too large for a room.

It is necessary to mention that objects exhibited must be in illustration of some communication (however short) to one of the sections, in order that they may thus be sanctioned by the committee of such section.

By the co-operation of the sectional secretaries with the members of the committee appointed to superintend the arrangements of this room or repository, we ought to succeed in adding an important and valuable feature to the scientific interest of the meetings of the Association. E. RAY LANKESTER

A Waterspout at Milford Haven

THE enclosed account of a waterspout which was sent to me by one of our telegraphic reporters may perhaps be of interest to your readers. ROBERT H. SCOTT

Aug. 1

"St. Ann's Head, Milford Haven, July 28

"Sir,—The waterspout mentioned in this morning's report was observed yesterday at 4.50 P.M., about a mile outside the port, following in the wake of a squall. Its course lay about N.E., and the progressive movement was judged to be between twenty-five and thirty knots per hour. Its diameter at the base was about 40 ft., and the direction of the whirl from left to right, or with the hands of a watch. The lower portion was well defined, but the middle and upper portions were not so distinct; in fact, the connection with the clouds above, although undoubtedly existing, could not be discerned from our point of view. The sea immediately under it was greatly agitated and white with foam, the spray ascending in a spiral form. Thunder was heard with the squall that preceded it, and the wind veered from S. to S.S.W., although it backed to S. again afterwards. (Signed) JOHN C. WALKER

"R. H. Scott, Esq."

Periodicity of Rainfall

MY attention has been recalled to the letter (vol. viii. p. 547) of my old friend Mr. Meldrum, dated Sept. 15 last, upon the above subject, by its recent republication in a Barbados newspaper. I had intended at the time to examine whether his objections to my statements were valid, but absence from the island and other occupations interfered. On reperusing his letter, I perceive that he notices a disagreement between my figures and those given by Mr. Symons, which requires to be explained, and I take the opportunity of endeavouring to remove his doubts with regard to the correctness of my results. Mr. Symons's annual averages for 1843–61 were drawn from one station, or rather from two; from Fairfield for the years 1843–46, and from Halton, a station nearly three miles distant, and having twice the elevation, for the rest of the period. My averages were taken for the first four years from the same single station, the only record then in existence, and from a varying number of stations during the other years.

Mr. Meldrum thinks that, with certain alterations which he suggests, my calculations will support his theory. I should be very glad if they did. My object in pursuing my inquiries into the rainfall of Barbados has been to assist the planters in forecasting the coming seasons, so as to guide them in their agricultural operations; and I would gladly welcome every contribution to this end, whether it be Mr. Meldrum's sun-spots or Prof. Chase's lunar influences. I was therefore disappointed when I found that the experience of this island did not coincide with that of Mauritius, and I am sorry that a further comparison of the data, which is not open to any objection of discordance of elements, confirms my first calculations.

If I take Fairfield and Halton alone, for the thirty-one years 1843–73, I obtain the following results:—

	Maximum years.	Minimum years.
1843-45 ...	... —	163·7
1847-49 ...	... 158·3	—
1855-57 ...	... —	170·7
1859-61 ...	... 186·6	—
1866-68 ...	... —	177·8
1870-72 ...	... 157·1	—
Total ...	502·0	512·2

This calculation shows an annual average excess in *minimum* years of 3·4 inches. But the rainfall at Fairfield during the last three years, for which alone I have the means of comparison, is 13·33 per cent. below that of Halton. Therefore 21·7 inches have to be added to the minimum average of 1843–45, which would increase the above excess to 10·6 inches. If Halton alone be taken for the five periods, the average of the maxima is 167·3, and that of the minima 174·2, yielding an excess of *minima* of 6·9 inches.

A comparison of three stations for 19 years, 1855–73, being the longest comparable period, exhibits the same results. These three stations, Halton, Binfield, and Husbands, are situated in opposite parts of the island, and furnish a fair average of the whole:—

	Maximum years.	Minimum years.
1855-57 ...	... —	192·7
1859-61 ...	... 193·6	—
1866-68 ...	... —	182·6
1870-72 ...	... 162·7	—
Total ...	356·3	375·3

This calculation shows an annual average excess of 9·5 inches in *minimum* years, which differs only by 1·1 inch from the above corrected calculations founded on the returns of a single station.

Mr. Meldrum, in his letter of September, writes, that I have "taken 1846 and 1871 as middle maxima years [in my first paper I also took 1848], whereas 1849–72 are probably more correct." Mr. Meldrum is in error as to my having taken 1846 as